tion therefore whether the American medical profession shall permit to develop unchallenged that movement now grown so powerful in this country whereby non-medical men are elevated to positions of authority and responsibility in public health matters, which after all are medical matters. Without doubt many non-medical men may become expert health officers and discharge their duties to the communities which they serve in an intelligent manner. Can they be trusted in a crisis however and are we willing as physicians that a practise so fraught with danger be continued?

Finally how can we educate the great mass of people in this country who are engaging in all sorts of philanthropic enterprises which verge on medicine or which require some medical advice and assistance if all this work is to be prosecuted intelligently. These individuals are constantly turning to the medical profession for the solutions of knotty, difficult problems and indeed in no time in the history of this country have physicians had greater opportunities of directing broad, comprehensive charitable movements in the proper direction so that great sums of money shall be intelligently used for useful and beneficial objects. This education of the people in matters affecting their health can probably best be given in a museum of hygiene where models of all sorts of apparatus, collections of charts and statistical materials can be made available for study, where public lectures can be given on health topics, where experts in various lines can be consulted, where commissions can be formed for the investigation of special problems of public health. Such a museum would become a great center for education in hygiene and public health and prove of incalculable benefit to the community in which it might happen to be located.

The question as to which of these three needs should first be satisfied is not easy to answer and the answer will also vary according to the individual point of view of those of us who study the problems. They are here presented in what seems to me to be the logical arrangement. If possible let us first educate our medical students, then our officers of health, then the public. Should the order be changed however no great harm will result. Should this country be so fortunate as to see schools of hygiene attached to the medical departments of our universities properly endowed and aiming to satisfy all three needs, then indeed shall we be fortunate beyond the wildest dreams of the most enthusiastic student of the subject.

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MEASUREMENTS OF THE DISTANCES OF THE STARS[1]

FOR the lecture in honor and memory of Edward Halley, which it is my privilege to deliver this year, I have chosen an account of the persistent efforts made by astronomers to measure the distances of the fixed stars. For many generations their attempts were unsuccessful, though some of them led to great and unexpected discoveries. It is less than eighty years ago that the distances of two or three of the nearest stars were determined with any certainty. The number was added to, slowly at first, but afterwards at a greater rate, and now that large

[1] The "Halley Lecture" (slightly abridged), delivered at Oxford on May 20, by Sir F. W. Dyson, F.R.S., Astronomer Royal, and printed in the issue of *Nature* for June 3.

telescopes are available and photographic methods have been developed, we may expect that in the next few years very rapid progress will be made.

For many centuries astronomers had speculated on the distances of the stars. The Greeks measured the distance of the moon; they knew that the sun and planets were much further away, and placed them correctly in order of distance, guessing that the sun was nearer than Jupiter because it went round the sky in one year while Jupiter took twelve. The stars, from their absolute constancy of relative position, were rightly judged to be still more distant—but how much more they had no means of telling.

In 1543 Copernicus published "De Revolutionibus Orbium Cœlestium," and showed that the remarkable movements of the planets among the stars were much easier to understand on the hypothesis that the earth moved annually round the sun. Galileo's telescope added such cogent arguments that the Copernican system was firmly established. Among other difficulties which were not cleared up at the time one of the most important was this: If the earth describes a great orbit round the sun, its position changes very greatly. The question was rightly asked: Why do not the nearer stars change their positions relatively to the more distant ones? There was only one answer. Because they are so extremely distant. This was a hard saying, and the only reply which Kepler, who was a convinced believer in the earth's movement round the sun, could make to critics was "Bolus erat devorandus."

Although no differences in the positions of the stars were discernible to the naked eye, it might be that smaller differences existed which could be detected by refined astronomical measurements. To the naked eye a change in the angle between neighboring stars not more than the apparent diameter of the sun or moon should be observable. No such changes are perceived. The stars are—it may be concluded—at least two hundred times as distant as the sun. With the instruments in use in the seventeenth century—before the telescope was used for the accurate measurement of angles—angles one twentieth as large were measurable, and the conclusion was reached that the stars were at least four thousand times as distant as the sun. But no positive results were obtained. Attempts followed with the telescope and were equally unsuccessful. Hooke tried to find changes in the position of the star γ Draconis and failed. Flamsteed, Picard and Cassini made extensive observations to detect changes in the position of the pole star and failed. Horrebow thought he had detected slight changes in the position of Sirius due to its nearness in a series of observations made by Römer. He published a pamphlet, entitled "Copernicus triumphans," in 1727, but the changes in the position of Sirius were not verified by other observers, and were due to slight movements of Römer's instruments.

Thus in Halley's time it was fairly well established that the stars were at least 20,000 or 30,000 times as distant as the sun. Halley did not succeed in finding their range, but he made an important discovery which showed that three of the stars were at sensible distances. In 1718 he contributed to the Royal Society a paper entitled "Considerations of the Change of the Latitude of Some of the Principal Bright Stars." While pursuing researches on another subject, he found that the three bright stars—Aldebaran, Sirius and Arcturus—occupied positions among the other stars differing considerably from those assigned to them in the Almagest of Ptolemy. He showed that the possibility of an error

in the transcription of the manuscript could be safely excluded, and that the southward movement of these stars to the extent of 37′, 42′ and 33′—*i. e.*, angles larger than the apparent diameter of the sun in the sky—were established. He remarks:

> What shall we say then? It is scarce possible that the antients could be deceived in so plain a matter, three observers confirming each other. Again these stars being the most conspicuous in heaven are in all probability nearest to the earth, and if they have any particular motion of their own, it is most likely to be perceived in them, which in so long a time as 1800 years may show itself by an alteration of their places, though it be utterly imperceptible in a single century of years.

This is the first good evidence, *i. e.*, evidence which we now know to be true, that the so-called fixed stars are not fixed relatively to one another. It is the first positive proof that the distances of the stars are sensibly less than infinite. This, then, is the stage at which astronomers had arrived less than two hundred years ago. The stars are at least 20,000 or 30,000 times as distant as the sun, but three of the brightest of them are perceived to be not infinitely distant.

The greatest step in the determination of stellar distances was made by another Oxford astronomer, James Bradley. His unparalleled skill as an astronomer was early recognized by Halley, who tells how

> Dr. Pound and his nephew, Mr. Bradley, did, myself being present, in the last opposition of the sun and Mars this way demonstrate the extreme minuteness of the sun's parallax and that it was not more than 12 seconds nor less than 9 seconds.

Translated from astronomical language, the distance of the sun is between 95 and 125 millions of miles. Actually the distance is 93 million miles. The astronomer who so readily measured the distance of the sun entered on the great research which had baffled his predecessors—the distance of the stars.

The theory of the determination of stellar parallax is very simple: the whole difficulty lies in its execution, because the angles are so small that the slightest errors vitiate the results completely. Even at the present time with large telescopes, and mechanism which moves the telescope so that the diurnal movement of the stars is followed and they appear fixed to the observer in the field of the telescope, and with the additional help of photography, the determination of the parallax of a star requires a a good deal of care, and is a matter of great delicacy. But in Bradley's time telescopes were imperfect, and the mechanism for moving them uniformly to follow the diurnal rotation of the stars had not been devised.

This was in some ways very fortunate, as the method Bradley was forced to adopt led to two most important and unexpected discoveries. Every day, owing to the earth's rotation, the stars appear to describe circles in the sky. They reach the highest point when they cross the meridian or vertical plane running north and south. If we leave out all disturbing causes and suppose the earth's axis is quite fixed in direction, a star S, if at a great distance from the earth, will always cross the meridian at the same point S; but, if it is very near, its movement in the small parallactic ellipse will at one period of the year bring it rather north of its mean position and at the opposite period an equal amount south.

Bradley, therefore, designed an instrument for measuring the angular distance from the zenith, at which a certain star, γ Draconis, crossed the meridian. This instrument is called a zenith sector, and is shown in the slide. The direction of the vertical is given by a plumb-line, and he measured from day to day the angular distance of the star from the direction of the vertical. From December, 1725, to March,

1726, the star gradually moved further south; then it remained stationary for a little time; then moved northwards until, by the middle of June, it was in the same position as in December. It continued to move northwards until the beginning of September, then turned again and reached its old position in December. The movement was very regular and evidently not due to any errors in Bradley's observations. But it was most unexpected. The effect of parallax—which Bradley was looking for—would have brought the star furthest south in December, not in March. The times were all three months wrong. Bradley examined other stars, thinking first that this might be due to a movement of the earth's pole. But this would not explain the phenomena. The true explanation, it is said, although I do not know how truly, occurred to Bradley when he was sailing on the Thames, and noticed that the direction of the wind, as indicated by a vane on the mast-head, varied slightly with the course on which the boat was sailing. An account of the observations in the form of a letter from Bradley to Halley is published in the *Philosophical Transactions* for December, 1728:

> When the year was completed, I began to examine and compare my observations, and having pretty well satisfied myself as to the general laws of the *phenomena*, I then endeavored to find out the cause of them. I was already convinced that the apparent motion of the stars was not owing to a nutation of the earth's axis. The next thing that offered itself, was an alteration in the direction of the plumb-line, with which the instrument was constantly rectified; but this upon trial proved insufficient. Then I considered what refraction might do, but here also nothing satisfactory occurred. At last I conjectured that all the *phenomena* hitherto mentioned, proceeded from the progressive motion of light and the earth's annual motion in its orbit. For I perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction, than that of the line passing through the eye and the object; and that, when the eye is moving in different directions, the apparent place of the object would be different.

This wonderful discovery of the aberration of light is usually elucidated by the very homely illustration of how an umbrella is held in a shower of rain. Suppose the rain were falling straight down and a man walking round a circular track: he always holds the umbrella a little in front of him—because when he is walking northward the rain appears to come a little from the north, when he is going eastward it appears to come a little from the east, and so on.

Although the phenomena Bradley had observed were almost wholly explained in this way, there were still some residual changes, which took nineteen years to unravel; and he explained these by a nutation or small oscillation of the earth's axis, which took nineteen years to complete its period. I can not dwell on these two great discoveries. For our present purpose, it should be said that aberration and nutation cause far greater changes in the apparent positions of the stars than, we now know, are caused by parallax. Until they were understood and allowed for or eliminated, all search for parallax must have been in vain. Further, Bradley's observations showed that in the case of γ Draconis, at any rate, parallax did not displace the star by so much as $1.0''$ from its mean position, or that the star was 200,000 times as distant as the sun. We may say that Bradley reached to just about the inside limit of the distances of the nearer stars.

Let me now try to give some idea of what is meant by a parallax of $1''$, which corresponds to a distance 200,000 times that of the sun. Probably many of you have looked at the second star in the tail of the Great Bear, Mizar, it is named, and have seen

there is a fainter star near it, which you can see nicely on a fine night. These stars are 600″ apart; with a big telescope with a magnification of 600 times—and this is about as high a magnification as can be generally used in England—two stars 1″ apart are seen double just as clearly as Alcor and Mizar are seen with the naked eye. I think this is the most useful way to think of 1″—a very small angle, which one needs a magnification of 600 times to see easily and clearly. Bradley showed that γ Draconis did not wander by this amount from its mean position among the stars in consequence of our changing viewpoint.

The next attempt to which I wish to refer is the one made by Sir William Herschel. In a paper communicated by him to the Royal Society in December, 1781, he reviews the serious difficulties involved in determining the parallax of a star by comparing its zenith distance at different times of the year. Especially there is the uncertainty introduced by the refraction of light, and in addition as the angular distances of stars from the zenith are changed by precession, nutation and aberration, any errors in the calculated amount of these changes will all affect the results. He proposed, therefore, to examine with his big telescope the bright stars and see which of them had faint stars near them. The bright stars, he said, are probably much nearer than the faint stars; and if the parallax does not even amount to 1″ the case is by no means desperate. With a large telescope of very great perfection it should be possible to detect changes in the angular distance of two neighboring stars. By this differential method the difficulties inherent in the method of zenith distances will be eliminated. Herschel made a great survey to find suitable stars, and in this way was led to the discovery of double stars—*i. e.*, of pairs of stars which are physically connected and revolve around one another, just like sun and earth. This was a most important discovery, but as the two components of a double star are practically at the same distance from us they do not serve to determine parallax, for which we need one star to serve as a distant mark.

For another forty years persistent efforts were made without success. Piazzi, in Italy, thought he had detected parallax in Sirius and a number of other bright stars, but the changes he detected in the zenith distances were unquestionably due to errors introduced by uncertainty in refraction, or slight changes in the position of his instruments in the course of the year. Dr. Brinkley, in Dublin, made a gallant effort and took the greatest pains. He thought he had succeeded, and for many years there was a controversy between him and Pond as to whether his results were trustworthy. The state of knowledge of the distances of the fixed stars in 1823 is summed up accurately by Pond in the *Philosophical Transactions:*

> The history of annual parallax appears to me to be this: in proportion as instruments have been imperfect in their construction, they have misled observers into the belief of the existence of sensible parallax. This has happened in Italy to astronomers of the very first reputation. The Dublin instrument is superior to any of a similar construction on the continent; and accordingly it shows a much less parallax than the Italian astronomers imagined they had detected. Conceiving that I have established, beyond a doubt, that the Greenwich instrument approaches still nearer to perfection, I can come to no other conclusion than that this is the reason why it discovers no parallax at all.

Besides these and other efforts to find parallax in the zenith distances of stars, attempts were also made to detect changes in the time at which the stars cross the meridian, to see if they are slightly before their time at one period of the year and slightly after it at another. But these, too,

were unsuccessful, even in the hands of astronomers like Bessel and Struve. The best were some observations of circumpolar stars made by Struve in Dorpat between 1814 and 1821. The following table shows some of the results at which he arrived:

Polaris and ϵ Urs. Maj.	$\pi + 0.053\pi' = +0.''075 \pm 0.''034$
ϵ Urs. Maj. and α Cass.	$\pi + 0.962\pi' = -0.136 \pm 0.110$
ζ Urs. Maj. and δ Cass.	$\pi + 1.099\pi' = +0.175 \pm 0.127$
β Urs. Min. and α Persei	$\pi + 0.402\pi' = +0.305 \pm 0.071$
Capella and β Drac.	$\pi + 1.147\pi' = +0.134 \pm 0.139$
β Aurig. and γ Drac.	$\pi + 1.138\pi' = +0.020 \pm 0.117$

This table has the merit of not looking wildly impossible in the present state of our knowledge. It has the disadvantage of not giving a definite parallax to each star. For example, it is impossible to say how much of the 0.134″ is to be given to Capella and how much to β Draconis. Further, the probable errors, though really small, are nearly as large as the quantities determined.

Struve and Bessel therefore attempted the problem by the differential method recommended by Herschel. By this time it had become easier to carry out. The method of mounting telescopes equatorially had been devised, so that the telescope was always kept pointing to the same part of the sky by clockwork-driven mechanism. Struve chose the bright star α Lyræ, and measured its distance from a faint star about 40″ away on ninety-six nights between November, 1835, and August, 1838. In the focal plane of his telescope he had what is called a position micrometer. The micrometer contains two parallel spider-threads stretched on frames, and the frames are movable by screws until the position shown in the diagram is reached: the distance apart of the threads is known by the readings of the screw-heads. He found that α Lyræ had a parallax 0.262″ with a probable error $\pm$ 0.025″.

Bessel chose the star 61 Cygni as a likely star to be near the sun, and therefore to have appreciable parallax. 61 Cygni is not nearly so bright as α Lyræ, but has a very great angular movement or proper-motion among the stars. Bessel used an instrument called a heliometer. Like Struve's telescope, it was mounted so that it could be driven by clockwork to point always at the same star. The object-glass of Bessel's telescope was made by the great optician Fraunhofer, with the intention of cutting it in halves. Fraunhofer died before the time came to carry out this delicate operation, but it was successfully accomplished after his death.

Delicate mechanism was provided for turning the glass, and also for moving the two halves, the amount of movement being very accurately measured by screws. Each half gives a perfect image of any object which is examined, but the two images are shifted by an amount equal to the distance one-half of the lens is moved along the other. Thus when a bright star and faint star are looked at, one half of the object-glass can be made to give images S and s, and the other half S′ and s'. By moving the screw exactly the right amount s' can be made to coincide with S, and the reading of the screw gives a measure of the angular distance between the two stars. Bessel made observations on ninety-eight nights extending from August, 1837, to September, 1838. The table, taken from a report by Main,[2] shows how closely the mean of the observations for each month accords with the supposition that the star has the parallax 0.369″:

[2] *Mem. R. A. S.*, Vol. XII., p. 29.

1837

Mean Date	Observed Disappointment	Effect of Parallax 0″369
August 23	+ 0.″197	+ 0.″212
September 14	+ 0.100	+ 0.100
October 12	+ 0.040	— 0.057
November 22	— 0.214	— 0.258
December 21	— 0.322	— 0.317

1838

Mean Date	Observed Disappointment	Effect of Parallax 0″369
January 14	— 0.″376	— 0.″318
February 5	— 0.223	— 0.266
May 14	+ 0.245	+ 0.238
June 19	+ 0.360	+ 0.332
July 13	+ 0.216	+ 0.332
August 19	+ 0.151	+ 0.227
September 19	+ 0.040	+ 0.073

Simultaneously with these determinations of the distance of α Lyræ and 61 Cygni, the distance of α Centauri, one of the brightest of the southern stars, was found by Henderson from observations of zenith distance made by him at the Cape between April, 1832, and May, 1833. He learned just before the termination of his residence at the Cape that this star had a very large proper-motion. Suspecting a possible parallax, he examined the observations when he had taken up his new office of Astronomer Royal for Scotland, and found a parallax amounting to 0.92″. He did not, however, publish his results until he found that they were confirmed by the right ascensions. In a communication to the Royal Astronomical Society in December, 1838, he states that it is probable that the star has a parallax of 1.0″.

The great and difficult problem which had occupied astronomers for many generations was thus solved for three separate stars in 1838 (see table).

Henderson's observation is interesting because α Centauri is, as far as we yet know, the nearest of all the stars to us. But by far the most valuable of these observations is Bessel's. The heliometer, which he devised, proved itself to be by far the most serviceable instrument for determining stellar parallax until the application of photography for this purpose.

	Parallax	Distance	Modern Observations	
			Parallax	Distance
α Centauri (Henderson)	1.″0	200,000	0.″750	270,000
61 Cygni (Bessel)	0.314	640,000	0.285	700,000
α Lyræ (Struve)	0.262	760,000	0.10	2,000,000

(The unit of distance is that from the earth to the sun.)

The somewhat dramatic manner in which the distances of three stars were determined in the same year, after several centuries of failures, may have led to the hope that the range of many more stars would soon be found. This was not the case, however. Each star had to be measured separately, and involved many nights of observations. The quantities to be measured were so small that they taxed the resources of the best instruments and best observers. In 1843 Peters published the parallaxes of half a dozen stars determined with the vertical circle at Pulkova, but the parallax of only one of these, Polaris, is obtained with much accuracy. With Bessel's heliometer, Schlüter and Wichmann measured the distance of Gr. 1830, the star which had the largest known proper-motion. In the 'sixties, Auwers with the same instrument determined the parallax of several quick-moving stars, and also of the bright star Procyon. With the Bonn heliometer, Krueger in the 'sixties measured the distance of three stars, and Winnecke two more. Other observations were made, amongst others, by Maclear, Otto Struve, Brünnow and Ball; but as these observers had not such suitable instruments, their results were not of the same high standard of value. A generous estimate would place the number of stars the distance of which had been satisfactor-

ily determined before 1880 at not more than twenty.

In the 'eighties, progress became more rapid. Gill, the Astronomer Royal for the Cape, in conjunction with a young American astronomer, Elkin, determined with great accuracy, though with only a small 4-inch heliometer, the distance of nine stars of the southern hemisphere. These stars included α Centauri, and the bright stars Sirius and Canopus. These results were communicated to the Royal Astronomical Society in 1884. The work of Gill and Elkin did not stop there. After some years, a very fine 7-inch heliometer was obtained at the Cape, and with it, between 1888 and 1898, the parallaxes of seventeen stars were determined by Gill and his assistants with very great accuracy. The stars observed at the Cape consisted of the brightest stars of the southern hemisphere, and of the stars with the greatest proper-motions. The results were remarkable. The stars with large proper-motions were nearly always comparatively near—say within one million times the sun's distance. On the other hand, some of the very brightest stars, particularly Canopus, the brightest star in the sky after Sirius, were at vastly greater distances.

Meanwhile Elkin, who had been appointed director of the Yale Observatory in 1884, carried out with a 6-inch heliometer, between the years 1885 and 1892, a determination of the distances of the ten brightest stars of the northern hemisphere. After these were finished the Yale observers, Elkin, Chase and Smith, embarked on the ambitious program of the determination of the distances of 163 stars of the northern hemisphere which show large proper-motion. They have added forty-one southern stars to these, and thirty-five stars of special interest. The results of all these observations were published in 1912. They have not, in most cases, the high accuracy of the Cape observations, but, nevertheless, are of great accuracy, and appear to be free from any considerable systematic error. A third important series of observations was made by Peter with a 6-inch heliometer at Leipzig. These were commenced about 1890, and continued until the death of Professor Peter in 1911. The parallaxes of twenty stars were determined with the same high accuracy as the Cape observations.

Observations with the heliometer require both skill and industry. To secure the needful accuracy measures must be made in four different positions of the instrument, so that possible small systematic errors may be eliminated by reversal. Great care is required in the adjustments of the instrument, particularly in the accurate determination of the scale-value at different temperatures. The possibility of obtaining satisfactory results with less labor was considered by Kapteyn, in view of the successful determination of the parallax of Gr. 34 by Auwers. From 1885 to 1887 he made observations with the transit-circle at Leyden of fifteen stars for the purposes of determining parallax. The observation consisted in observing the time when the star the parallax of which was sought and two or three neighboring stars crossed the meridian. Observations are made at the two most favorable epochs—say every night in March, and every night in September—to determine whether the star has changed its position relatively to its neighbors in the interval. The difficulties are twofold. The purely accidental error of observations of transits is considerable as compared with the small quantity which is sought. Besides this, the star of which the parallax is required is probably brighter than the comparison stars, and special precautions are required to guard against personal errors of the observer.

In questions of this kind the only satisfactory way is to judge by the results. From observations made on fifty nights, values of the parallax are obtained not nearly so accurate as the best heliometer observations, but still of considerable accuracy. Finally, the parallaxes of four of the stars which had been previously determined by measures with a heliometer showed satisfactory agreement.

This method has been employed by Jöst at Heidelberg, very extensively by Flint at the Washburn Observatory of the University of Wisconsin, and is now being tried at the Cape by Vouté, a pupil of Kapteyn's. It appears to me that this method can never give results of the highest accuracy, but that it may be of use in a preliminary search for stars of large parallax. The argument of the facility of the method compared with the heliometer has, however, lost much of its force; for, as I hope to show next, the highest accuracy attainable with the heliometer can be secured much more easily with a photographic telescope.

The application of photography to the determination of stellar parallax was first made by Pritchard in Oxford between 1887 and 1889. He took a large number of photographs and measured on them the angular distance of the star which he was considering from four of its neighbors. In this way he determined the parallax of five stars. He began this work late in life, and it was left for others to develop the photographic method and find what accuracy could be attained with it. At first sight it seems very easy, but experience shows that there are a number of small errors which can creep in and vitiate the results, unless care is taken to avoid them.

It has gradually become clear that with a few simple precautions and contrivances, a greater accuracy can be reached in the determination of parallax by photography and with much less trouble than by any other method. Between 1895 and 1905, several astronomers succeeded in obtaining from a few plates results as accurate as could be obtained from many nights' observations with the heliometer by the most skilled observers. In the last five years a large number of determinations have been made. In 1910 Schlesinger published the parallaxes of twenty-five stars from photographs taken with the 40-inch refractor of the Yerkes Observatory, and in 1911 Russell published the parallaxes of forty stars from photographs taken by Hinks and himself at Cambridge. The opinion expressed by Gill on these observations[3] was that but for the wonderful precision of the Yerkes observations, the Cambridge results would have been regarded as of the highest class. The facility with which the Yerkes results are obtainable is expressed very tersely by Schlesinger:

> The number of stellar parallaxes that can be determined per annum will in the long run be about equal to the number of clear nights available for the work.

With the heliometer at least ten times as much time would have been required. During the last year two further installments of the results of the Yerkes Observatory have been published by Slocum and Mitchell, giving the parallaxes of more than fifty stars. It might be thought that the high accuracy attained by them is largely attributable to the great length of the telescope. From experience at Greenwich, I do not think this is the case, and believe that similar results are obtainable with telescopes of shorter focal length. As several observatories are now occupied with this work, we may expect that the number of stars the distances of which are fairly well known will soon amount to thousands, as

[3] *M. N.*, Vol. LXII., p. 325.

compared with three in 1838, about twenty in 1880, about sixty in 1900, and now perhaps two hundred.

The stars the distances of which have been measured have generally been specially selected on account of their brightness or large proper-motion. Each star has been examined individually. Kapteyn has suggested that instead of examining stars singly in this way, photography gives an opportunity of examining all the stars in a small area of the sky simultaneously, and picking out the near ones. The method has been tried by Kapteyn and others—among them Dr. Rambaut. The idea is very attractive, because it examines the average star and not the bright star or star of larger proper-motion. It is liable, however, to some errors of systematic character, especially as regards stars of different magnitudes. Comparison of the results so obtained with those found otherwise will demonstrate whether these errors can be kept sufficiently small by great care in taking the photographs. Until this is done no opinion can be expressed on the success of this experiment, which is worth careful trial.

The question may be asked, How near must a star be to us for its distance to be measurable? I think we may say ten million times the sun's distance. This corresponds to the small angle 0.02″ for the parallax. If a star's parallax amounts to this, there are, I believe, several observatories where it could be detected with reasonable security, though we shall know more certainly by the comparison of the results of different observations when they accumulate.

You will readily imagine that an accurate knowledge of the distances of many stars will be of great service to astronomy. There are ample data to determine the positions, velocities, luminosities and masses of many stars if only the distances can be found. Thus we know the distance of Sirius, and we are able to say that it is travelling in a certain direction with a velocity of so many miles per second; that it gives out forty-eight times as much light as the sun, but is only two and a half times as massive. The collection and classification of particulars of this kind is certain to give many interesting and perhaps surprising results. But it is not my purpose to deal with this to-night. The task I set before myself in this lecture was to give an idea of the difficulties which astronomers have gradually surmounted, and the extent to which they have succeeded in measuring the distances of the stars.

F. W. Dyson

SCIENTIFIC NOTES AND NEWS

Fifty years ago William North Rice was graduated from Wesleyan University, and two years later was elected professor of geology and natural history, a title which was changed to professor of geology in 1884, when the department of biology was established. Professor Rice's services as teacher, administrator and investigator were acknowledged by the conferring on him of the degree of doctor of laws by Wesleyan University at its recent commencement.

Dr. Victor C. Vaughan, professor of hygiene at the University of Michigan and president of the American Medical Association, received the honorary degree of LL.D. at the annual commencement of Jefferson Medical College, Philadelphia, on June 5.

Three doctorates of science were conferred by the University of Pennsylvania at its commencement exercises on June 16. The recipients and Provost Smith's remarks were as follows: *Robert Andrews Millikan*—Physicist of eminence, editor, whose investigations in electricity, in molecular physics and heat have won for you deserved and well-merited recognition. *Harry Frederick Keller*—Because of your profound knowledge of chemical science,